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THE USE AND ABUSE OF THE IONIC THEORY[1]

TWENTY-FIVE years have elapsed since Arrhenius advanced the theory that acids, bases and salts in aqueous solution are dissociated into their constituent ions. Now that the storm of contention aroused by this doctine is clearing, it may not be inappropriate to consider in cooler blood this proposition of Arrhenius, to reinspect the foundations, and to weigh without prejudice the pros and cons, the successes and failures of the ionic theory.

To show that an electrolyte in solution suffers a change analogous to dissociation, Arrhenius brought forward evidence of three different kinds. First, he pointed out that the various methods of determining molal concentration in solution (freezing-point, boiling-point, vapor pressure, osmotic pressure), all of which are identical in principle and yield nearly identical results, indicate that in a salt solution the number of molecules dissolved, or less hypothetically the number of mols, is greater than the number calculated from the simple chemical formula of the salt.

The second argument rests upon the observation that in an aqueous solution of a strong electrolyte the properties are purely additive. Thus a dilute solution of hydrochloric acid has no properties which are peculiarly its own. It tastes sour, turns litmus red, dissolves metals, inverts sugar and possesses a number of other well-known properties, all of which are possessed in some degree by every acid. Moreover, it precipitates silver and mercurous salts, and

[1] Address of chairman of the Section of Physical Chemistry, Baltimore, December 29, 1908.

exhibits other properties which are found in all chloride solutions. In other words, the solution has no properties which are not included in one of two distinct sets, one possessed by all acids, and one by all chlorides. So it seems natural to regard this solution as a mixture of two substances, hydrogen ion, which is present in all acids, and chloride ion, which is present in all chlorides. To illustrate this additive property let me perform this simple and familiar experiment. Here are two solutions in alcohol, one containing cobalt nitrate, the other an equivalent amount of cobalt chloride. One is red, the other a brilliant blue. On pouring these two into equal large volumes of water, the difference in color disappears and both assume the pink color which is typical of aqueous cobalt solutions. Here again are three copper salts, nitrate, chloride and bromide, which in alcohol are respectively blue, green and dark brown, but when poured into equal volumes of water all show the same blue color of the cupric ion.

Finally, the third main argument in favor of ionization is derived from the electrical properties of solutions. Some fifty years ago Clausius believed that the conduction of electricity in electrolytes affords sufficient evidence to show a dissociation into ions. Whether or not we accept this proof, which is perhaps a little metaphysical, other experimental facts such as Kohlrausch's law of the additivity of conductivities at infinite dilutions, the agreement between conclusions drawn from conductivity and transference experiments, and the coincidence in the degree of dissociation calculated from conductivity and from freezing point, all give strong support to the theory of ionic dissociation.

Unfortunately, some over-enthusiastic advocates of the ionic theory, not content with this solid evidence, have superimposed on the theory other extraneous speculations which, when later discredited, have in some quarters brought the parent theory into disrepute. For example, I may mention the *dictum* that all chemical reactions in aqueous solutions are ionic in character, a notion which not only is intrinsically improbable upon theoretical grounds, but has been refuted experimentally by the experiments of Kahlenberg and others. Some too zealous ionists have applied the theory to highly concentrated solutions, without making allowances for the deviations from the laws of the perfect solution which are to be expected there. It would be absurd to class as a dilute solution one which is five or ten times molal, yet we see attempts to apply to a pair such as sodium and potassium nitrates the law of solubility lowering which has been obtained for ideal solutions.

In justice to the author of the ionic theory, it should be noted that he has had no part in these attempts to stretch the theory beyond its elastic limit. In his papers on this subject, brief as they have been, Arrhenius has with great fairness and extraordinary acumen stated, as far as our present limited knowledge permits, the truth, the whole truth, and nothing but the truth about ionic dissociation.

Against the excess of zeal in some advocates of the theory may be balanced the dogmatism of others who for *a priori* reasons have declared it absurd that a substance like potassium chloride, bound together presumably by an enormous affinity, could break spontaneously into its constituent parts. Instead of attempting to refute such circular reasoning, let us return rather to the consideration of that experimental material upon which are based the three main arguments for the ionic theory. Here a careful scrutiny reveals facts which, disconcerting as they may be, no fair advocate of ionic dissociation can afford to ignore.

The first of these unpleasant facts is that the values for the degree of dissociation of strong electrolytes calculated on the one hand from freezing points, and on the other from conductivities, while usually fairly concordant, frequently differ by an amount far greater than the experimental error. For half normal solutions of lithium chloride, magnesium chloride and calcium ferrocyanide, the degrees of dissociation calculated from the freezing points are 94 per cent., 99 per cent. and 2 per cent., while from conductivities we calculate 71 per cent., 62 per cent. and 20 per cent., respectively. Of course these are moderately concentrated solutions and at higher dilutions the discrepancies become less. Moreover, it is not unlikely that the attempts to explain such facts, by assumptions of hydration, association, and the like, may ultimately be successful, but in the meantime these facts can not be neglected.

In the second place, the additivity of the properties of electrolytic solutions, striking as it is, seems to prove too much. If it is an argument for the dissociation of electrolytes, it seems to be an argument for complete dissociation. Why should the properties of a normal solution of potassium chloride be simply those of potassium and chloride ions if, as measurements of conductivity show, it is 25 per cent. undissociated? Why should the undissociated part have no individual properties of its own? It is easy to see why completely dissociated acids and bases should give the same heat of neutralization, since we regard this heat as simply due to the union of hydrogen and hydroxide ions, but half-normal potassium and sodium hydroxides give essentially the same heat of neutralization with an acid, although they are 20 per cent. undissociated. Half-normal barium hydroxide gives the same, although 40 per cent. undissociated. Copper sulphate as dilute as one tenth normal is still more than half undissociated, but its color is nearly the pure color of cupric ion. Indeed in all the strong electrolytes the partial volume, heat capacity, internal energy, viscosity, refractive index, rotary power, in fact practically all the significant physical properties of the undissociated part of the electrolyte, seem practically identical with the properties of the constituent ions. If we had no other criterion for the degree of dissociation, these facts would undoubtedly lead us to regard salts, up to a concentration of normal or half normal, as completely dissociated.

Finally, the phenomena of electrical conduction present several puzzling, and as yet unexplained, features. For example, attention has recently been called to the interesting fact that the two ions which in aqueous solution possess by far the greatest mobility, are the ions of water itself, hydrogen and hydroxide. This might possibly be regarded as chance if it had not also been found that in other solvents a similar condition exists. Thus in methyl alcohol, the methylate ion moves with unusual velocity. To explain this curious fact, it has been suggested that the ions of the solvent have a mode of progress different from that of other ions, due to their ability to pass *virtually* through the molecules of solvent. This view, in a certain sense, requires a return to a modified Grotthus theory, and if accepted, necessitates the conclusion that the process of conduction is not quite so simple as it may have seemed to the original advocates of the ionic theory.

Perhaps the most vulnerable point in the whole armor of the ionist is reached when we attempt to apply the mass law to the dissociation of strong electrolytes. The mass law derived rigorously only for the perfect solution could hardly be expected

to be exactly true in the case of actual solutions. We might therefore expect certain small deviations from the mass law, but are in no way prepared for the startling discrepancies which are in fact observed. This discrepancy is sufficiently marked in the case of salts of the simplest type, like potassium nitrate or sodium chloride, but is most striking in the case of some salts of a higher type. The following table gives for three electrolytes the values of the "mass law constant" (K) at different molal concentrations (C). We see that when the concentration changes one thousand fold, K changes one hundred fold in the case of potassium chloride and one million fold in the case of potassium ferrocyanide! For the weak electrolyte, acetic acid, it is a real constant.

Acetic Acid		KCl		$K_4Fe(CN)_6$	
C	K	C	K	C	K
.001	.00177	.0001	.0075	.0005	.7
.004	.00180	.001	.035	.002	18.0
.01	.00179	.01	.132	.012	1171.0
.02	.00179	.1	.495	.1	41190.0
.1	.00180	1.0	2.22	.4	842100.0

This extraordinary divergence from the mass law, of which I have chosen the most extreme case known in aqueous solution, is, however, found to an even more startling degree in the case of non-aqueous solutions. To the extremely bizarre conductivity curves there obtained few have had the temerity to apply in full the principles derived from the ionic theory. Nevertheless, we are beginning to realize that the phenomena of aqueous solutions are but special instances of the widely varying phenomena occurring in other solvents; and it seems unlikely that a satisfactory understanding of the behavior of aqueous solutions can come except through a careful study of non-aqueous solutions. At present our quantitative knowledge of such solutions is extremely limited, and does not encourage the belief that we have in any sense a final answer to the problem of solutions.

These, then, are some of the weak points of the ionic theory as it stands to-day. If the case were to rest here I am afraid it would be difficult to bring in a verdict for the theory of dissociation. Indeed many scientists, on the basis of such evidence, have decided to close the hearing and to class the ionic theory with other ingenious hypotheses that have failed to stand the test of experience. But these men have not applied the one criterion by which in the end every scientific proposition must be judged—the test of serviceability. After all, what have we said except that the ionic theory is not complete? But perfection is rare in the science of chemistry. Our scientific theories do not, as a rule, spring full-armed from the brow of their creator. They are subject to slow and gradual growth, and we must candidly admit that the ionic theory in its growth has reached the "awkward age." Instead, however, of judging it according to the standard of perfection, let us simply ask what it has accomplished, and what it may accomplish in scientific service.

When we examine a little more critically the unfavorable features which we have mentioned, we find that they enter chiefly in the application of the theory to strong electrolytes. If we consider the weak electrolytes, like ammonia, acetic acid and most of the organic substances, of which a large number have been investigated, we find a remarkably satisfactory state of affairs. For these the mass law has generally been shown to hold with remarkable accuracy. Indeed it is hardly too much to say that every prediction from the ionic theory has been quantitatively verified for all the weak electrolytes which have been carefully investigated. The degrees of dissociation ob-

tained in different ways are in complete accord; the properties are additive only just in as far as the electrolytes are ionized; the electrical properties seem entirely normal. Here, then, is an enormous field in which the whole theory of Arrhenius may be quantitatively applied, with perfect safety, to a wide variety of problems.

Again, in the case of strong electrolytes at high dilution, the theory of ionic dissociation is completely in accord with all known facts. The agreement between transference numbers measured directly and those calculated from the conductivities at infinite dilution is eminently satisfactory. There can hardly be any question that with increasing dilution the ratio of the molal concentration to that calculated when no dissociation is assumed approaches just two for binary, and just three for ternary salts. Is there any other hypothesis which will account for this cardinal fact?

Any valid criticism of the ionic theory must, therefore, be based upon its application to solutions of strong electrolytes at moderately high concentrations. Here the problem is unquestionably one of great difficulty. Even the simple question of determining the true degree of dissociation is one which still permits much divergence of opinion. One of the suggestions which has been made to account for some of the anomalies of the strong electrolytes is that, owing to the change in the mobility of the ions with the concentration, the conductivity is not a correct measure of the degree of dissociation. In some cases, especially in the case of the hydrogen ion, this supposition has indeed been definitely verified. Yet in the majority of cases it seems very unlikely that we may thus explain the extraordinary discrepancies such as those that I have pointed out in the application of the mass law to strong electrolytes. I have shown in another place that while conductivity may not indeed be an absolutely reliable measure of the concentration of the ions, it furnishes, nevertheless, the only way that we have of determining them. Every other method which has been used gives a measure, not of the real concentration, but rather of the escaping tendency, or activity, which may not in all cases be proportional to the concentration. I believe that we shall make no great error in assuming that the degree of dissociation calculated from the conductivities is in most cases substantially correct and that the lack of fulfilment of the requirements of the mass law for strong electrolytes is due to deviation of one or more of the dissolved substances from the laws of the perfect solution. This assumption is by no means inherently improbable. In fact, in the case of many non-electrolytes we find marked deviations from the ideal laws even at small concentrations.

If we must conclude that one of the substances present in a solution of an electrolyte is abnormal in its behavior, we are inclined at first to suspect the ions, which on account of their peculiar electrical condition might be expected to differ materially from ordinary substances. But this view proves to be untenable. As far as we can judge, the ions seem to obey, at least to a fair degree of approximation, the laws of the perfect solution, and thus we are forced to place the responsibility for the observed anomalies chiefly upon the undissociated part of the electrolyte.

The correctness of this idea may be tested by applying the simple laws of solutions to those cases where the properties of the undissociated substances may be eliminated from consideration. Every such test, which our present experimental material permits, substantiates this hypothesis. Thus, for example, when we consider the equilibrium between a dissolved salt and the ions in its saturated solution we need not consider the undissociated portion. Assuming that the

ions are normal in behavior, we are led at once to the principle of the constancy of the solubility product, the substantial correctness of which has been demonstrated by the experiments of Noyes, and the more recent work of Stieglitz. An entirely similar method which depended upon the elimination of the undissociated electrolyte was employed by Rothmund in his study of the dissociation of picric acid.

Another deduction which is similarly justified is that the product of the hydrogen and hydroxide ion concentrations is a constant, in any dilute aqueous solution, and this important constant has been obtained by several independent methods, all in excellent agreement. Finally the Nernst equation for the electromotive force of a concentration cell gives very satisfactory results when we consider only the ion concentrations. If, however, we apply an equation similarly obtained to the undissociated portion of the electrolyte we obtain results which are by no means corroborated by experiment.

A few years ago I had occasion to make a calculation which involved simultaneously the application of all the principles which I have just enumerated, the Nernst equation, the solubility product, the dissociation constant of water. By the aid of these it was possible to calculate from the decomposition pressure of silver oxide the potential of the oxygen electrode. The potential thus obtained differed more than one tenth of a volt from the value previously accepted, but was in perfect agreement with the results of the independent investigations published during the same year by Haber and by Nernst. The calculation would obviously have been vitiated if any one of the principles used had been unreliable.

To review the service rendered by these simple generalizations deduced from the ionic theory would be to summarize a very considerable part of the exact work in physical chemistry published during the past two decades. In the study of chemical equilibrium and reaction velocity, especially in the process of rationalizing quantitative analysis, these principles are of daily service.

While therefore many difficult problems relating to the application of the ionic theory remain to be solved, this theory must even at the present time be regarded as established on a sound working basis. Advance will come through the exact quantitative study of the properties of aqueous, and especially of non-aqueous, solutions. After this work is completed it is not improbable that our views of the nature of solutions will be greatly changed, but I venture to predict that the later and better theories will not be substitutes for, but rather developments of, the simple hypothesis of Arrhenius.

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THE COLLEGES OF THE UNITED STATES AND THE CAMPAIGN AGAINST TUBERCULOSIS[1]

The colleges and universities of the United States are social forces of such power that the campaign against tuberculosis can not ignore them: it needs their help, it seeks to enlist them among its strongest allies. And the campaign is so reasonable, so timely, and already so efficient, that it may confidently expect the cooperation of the colleges of the country, as, indeed, it depends on the cooperation of all intelligent men and women interested in the betterment of man.

First in importance among the aids which the higher schools are giving is perhaps the investigation of fundamental

[1] Read before the International Congress on Tuberculosis, Washington, D. C.

problems. It is largely in the universities that the problems which come under the cognizance of this section of the congress—problems of hygiene and sanitation, of economics and sociology—are being solved, and it is here that students of the subject are trained in special methods of research. As examples of various lines of investigation carried on in the higher schools, I may mention the advanced courses in social economics under Dr. E. T. Devine, at Columbia University, where in 1908 an entire term was devoted to seminar work on the social aspects and control of this disease; the investigations of Professors Fisher, Baily and Farnham, of Yale, in diet and housing in sanatoria, and in the relation of tuberculosis to labor and tenement conditions; the sociological work at the University of Chicago where students accompany patients of the college dispensary to their homes, together with the regular visiting nurses and physicians and study local conditions and surroundings and the methods employed in improving the environment; and the work of the University of Wisconsin whose classes from the departments of political economy and sociology visit Milwaukee to study the social and industrial aspects of the disease. In not a few colleges students are securing valuable data as to some of the simpler problems of the local fields of the college or of their home towns and counties, such as the number and proportion of deaths from tuberculosis, the recurrence of the disease in infected houses, the average length of the disease, and its economic losses. From answers to a questionnaire sent to two hundred representative higher schools of the United States, it is found that about one fifth are engaged in investigative work in tuberculosis.

The higher schools are also furnishing from their faculties not a few men of knowledge and conviction as leaders in the propaganda. The lists of officers of this congress, of the National Association and of the state leagues and state boards of health show that the colleges are supplying at least their full quota for this purpose.

A most effective help which the colleges are giving in the fight is along educational lines. In states where the commonwealth does not furnish lecturers for the educational campaign, the work of arousing and teaching the people from the platform falls largely on the schools. University extension courses, summer chautauquas and more incidental occasions furnish a means of reaching the people which Phillips and Garrison might have coveted in their campaign against slavery. The subject of public health receives an interested hearing everywhere, and on this theme the college man speaks with an authority and influence enhanced by his institutional relations. And to college men the educational campaign makes a specially strong appeal. We can not see the people perish for want of knowledge, knowledge which it happens to be our good fortune to possess, and not be stirred by some missionary zeal to go forth and preach the gospel of sanitation and the salvation which it offers from disease. University extension lectures on tuberculosis are now offered by at least fifteen colleges and universities.

A still more fruitful field lies within the college walls. In our students we find an exceptional receptivity to new truth. The stolidity of ignorance, unable to apprehend fundamental principles, the inertia of long-fixed habits of thought and will, the prejudice of financial interests imperiled—none of these obstacles are present in the college. Surely that warm-hearted enthusiasm of youth, easily stirred to noble ends, which in our civil war sent forth to battle the boys of the colleges of North and South

alike, may be aroused to serve in a campaign whose purpose is not to destroy life but to save it.

Some of our colleges are teaching by example the prophylaxis of the disease in the way of bacterial cleanliness. At Dartmouth the class rooms, dormitories and chapel are examined every two weeks by the exposure of gelatine plates, and when the number of bacteria which fall on these plates during ten minutes exceeds forty, the rooms are thoroughly disinfected with formaldehyde. Since the adoption of the wholesale method of disinfection the sickness among the students has been very markedly reduced, especially those mild forms of disease such as pharyngitis, tonsilitis, measles and ordinary colds. In a number of schools immediate attention is given to any suspicious cases of incipient tuberculosis. Colleges whose class rooms are well ventilated, which occasionally test them for the proportion of carbon dioxide present, teach the value of fresh air in the most emphatic way. And the schools whose aim is not the making of a few over-specialized athletes, but rather the physical well being, the maximum efficiency of all their students, inspire an ideal of vigorous health which translated into life becomes the best possible prophylaxis against tuberculosis.

As a matter of education, as well as for stronger reasons, a number of colleges have prohibited the rooming of students in families afflicted with tuberculosis and in houses where the occurrence of such cases has not been followed with disinfection. Where there are state or municipal regulations requiring registration and supervision of cases and prompt disinfection of houses after death or removal, and where these rules are rigidly enforced, there may be little need of college rules. But the replies to our questionnaire indicate that such municipal or state regulations obtain in less than half of college towns and in a still smaller number are they effectively enforced. Apparently in the majority of our higher schools no control is exercised on this vital matter. The young student comes to the college town a stranger, utterly ignorant of the sanitary conditions of the houses of the town among which he is to choose his home. Very possibly he is ignorant of the causes of tuberculosis and sees no danger in joining a family infected with it. He is thus allowed to place his life in hazard without even a remonstrance from those who are supposed to have his physical well being in their charge and who with no great difficulty can usually know of the special places which form the ambushes of the disease. The reason why such regulations are not more widely made is in part the general exemption of college students from serious diseases. But before this reason is held sufficient, an examination should be made into the mortality from tuberculosis of the younger alumni, a mortality which is often high, and which in some instances may possibly be found significant of conditions in the environment of the college. The following from a correspondent in one of the large universities of the middle west may represent the attitude of many schools:

> I believe that there are no faculty regulations at the present time, but the committee on hygiene has considered the matter and expects to inaugurate an active campaign in regard to this with the next school year. There has been so little trouble of this sort among university students that the need has not seemed urgent heretofore, but as matter of education I am personally very strongly in favor of it.

Among the matters which make for the education of the student as to tuberculosis is the use of the tuberculin test with the dairy cattle kept by agricultural colleges and by a number of schools for the use of

their boarding halls. The regular inspection of such dairy herds, reported by a number of colleges, is a duty so obvious that its neglect will go far to neutralize the most efficient teaching of the class room.

Our colleges are using the directest means of enlisting their students in the war against the great white plague. Local anti-tuberculosis societies are organized, as at Radcliffe. Special lectures or courses of lectures are given before student assemblies. At the Rose Polytechnic and at Bryn Mawr such addresses are given by the president. At Vanderbilt University the director of a tuberculosis exhibition held at Nashville was obtained to address the students. At Cornell College, Iowa, the Science Club for four successive years has provided open evening lectures for students and townspeople, securing for this purpose the state bacteriologist, the state lecturer on tuberculosis and one of the government meat inspectors from one of the cities of the state as well as physicians and teachers of the town and college. In about one fifth of the 200 representative colleges such lectures are now given before student assemblies.

A still larger number of schools give special attention to tuberculosis in the class room. At Dartmouth the physical director gives two or three lectures a year to the freshman class in a course on hygiene. Indiana State University provides instruction on tuberculosis, its causes, results and methods of prevention and cure, in a course of lectures on hygiene given by different members of the faculty and required of all candidates for graduation in the college of liberal arts. Purdue University assigns a large place to the subject in a series of lectures on health and efficiency and Carlton College provides for the same in a course on social problems. The propaganda is carried on in many schools in the departments of biology and hygiene, in sociology, domestic science, sanitary engineering, and economics. The University of Wisconsin for several years has given lectures on tuberculosis in farmers' courses and before various meetings of teachers. The University of Minnesota makes such lectures an integral part in the program of its college of education.

The recent organization of schools of education in the stronger colleges and universities opens a large field for the propaganda. Colleges and universities now supply the majority of high-school teachers. To teach the students of the schools of education in the colleges and universities the facts as to tuberculosis is to disseminate these facts throughout the secondary schools. To enlist college and university men and women in the great crusade is to draw under the same banner the hundreds of thousands of high-school boys and girls who in the immediate years are to be under their instruction.

An educational agency effectively employed in some schools is the circular. The students of the University of Minnesota were recently thoroughly circularized on the subject under the auspices of the anti-tuberculosis committee of the Associated Charities of Minneapolis. At Syracuse University the students have entered so heartily into the campaign that they recently distributed cardboard circulars on tuberculosis printed in five languages to the 25,000 homes of the city.

Perhaps less than one half of our higher schools have as yet actively interested themselves in the twentieth century crusade. Yet even those replies to our questionnaire which confess a total lack of co-operation in the present are often most encouraging in their promise for the future. "It is high time that we devoted some attention to a matter of such vital importance," writes the president of a col-

lege of the middle west. And this from a president of one of the state universities:

> I have to say in humiliation that practically nothing has yet been done in this state and in our institution as to tuberculosis. . . . I am going to take up the matter in the university next year. I regard the movement as one of supreme importance and hope to bear my share in the beneficent cause.

What results may we expect from the cooperation of all the colleges when it is once secured? Each year we shall enlist hundreds of thousands of the best young men and women of the country. We shall obtain through them the cooperation of the secondary schools whose teachers they supply. And as our students go out into every city and town and village in the land to take their places as citizens of exceptional influence in their communities, we shall secure the cooperation of the leaders of the future. The harvest sown in the college may be some years in ripening, but it is no less sure.

In summation let me say that we may reasonably ask the college and university to help us by giving instruction on tuberculosis in general lectures before student assemblies and in specific teaching in the class rooms of the social, economic and biologic sciences. We may ask for exemplary sanitation of college buildings, for inspection of college herds, and for care over the health of students by regulations securing immunity from house infection and by the early detection of incipient cases of the disease. We may ask for the exertion of effective influence in securing model sanitary conditions in the immediate college environment and for help in promoting the propaganda throughout the state and nation in every possible way.

W. H. Norton

Cornell College, Iowa

PROPOSED PUBLICATION OF EULER'S WORKS

Strenuous efforts are now being made to secure the publication of the complete works of Leonhard Euler (1707–83), one of the most prolific writers of all times on pure and applied mathematics. Euler lived at a time when the differential and integral calculus was still young, and he was most influential in making this powerful instrument of thought more easily available in the various fields of mathematics. The enormous extent of his writings has been a great obstacle in the way of securing a publication of his complete works and has thwarted earlier efforts along this line. From a recent circular issued by the Swiss Society of Natural Sciences, it appears that we may reasonably expect that the publication of this great work will begin at an early date. The following extract from this circular should be of interest:

> On the initiative of the German Association of Mathematicians, the International Mathematical Congress, meeting at Rome in April, 1908, unanimously passed the following resolution:
>
> "The fourth International Congress of Mathematicians, held in Rome, regards the publication of the whole collection of Euler's works as an undertaking of the greatest importance, both to pure and to applied mathematics. The congress gratefully welcomes the initiative taken by the Swiss Society of Natural Sciences in this matter and expresses the wish that the great work may be carried out by that society in common with the mathematicians of the other nations. The congress begs the International Association of Academies, and more particularly the Berlin and St. Petersburg academies, of which Euler was so preeminent a member, to support the enterprise in question."
>
> Immediately on the adoption of this resolution the representative of the Paris Academy, Mons. G. Darboux, made known that the International Association of Academies had discussed the Euler question at Vienna in the preceding year, and had expressed entire sympathy with the movement. The correspondence which has since taken place between the president of the Swiss Euler Committee and Mons. Darboux, as also with Herr Lindemann, who had aroused interest in the matter in Vienna, leads us to hope that the support of the Association of Academies will be

accorded to the Swiss Society of Natural Sciences, as desired at the congress in Rome.

At its annual meeting, on August 30, 1908, the Swiss Society of Natural Sciences passed the following resolution, proposed by the Central Committee:

§ 1. The Swiss Society of Natural Sciences is willing to publish a complete edition of Leonhard Euler's works, on condition that this undertaking be adequately supported by the government authorities of the Swiss Confederation and of the cantons, as well as by learned bodies and scientists, both at home and abroad, and that the scientific cooperation required for its accomplishment be forthcoming.

§ 2. The Swiss Society of Natural Sciences entrusts to the Euler Committee in cooperation with the Central Committee the execution of all preliminary work.

§ 3. On the conclusion of the preliminary work a further resolution on the part of the society will be necessary before proceeding to publication.

To § 2 some special remarks were added, the last of which demanded "the creation of a fund, by means of private contributions and subscriptions, for the eventual publication of Euler's works."

The Central Committee and the Euler Committee of the Swiss Society of Natural Sciences have always regarded it as a matter of course that the collection of voluntary contributions should begin in Euler's native land. And we are pleased to be able to announce that this idea has been cordially seconded in all parts of Switzerland. The list is not yet closed, but the subscription will most probably reach the amount of 100,000 francs ($20,000). We accordingly consider ourselves justified in turning now to other countries for their support.

Two distinguished scientific bodies have already set a praiseworthy example in this direction.

In September, 1908, the German Association of Mathematicians decided at their annual meeting at Cologne to hand over 5,000 francs to the Swiss Society of Natural Sciences for the publication of Euler's works. This amount is of great significance when we add that the German Association of Mathematicians was thus devoting one third of all its available funds to the object in question.

The resolution unanimously proposed by the managing committee and warmly recommended by the chairman, Professor Dr. Felix Klein, was accepted by the Cologne meeting in a spirit of noble enthusiasm, unanimously and without discussion. The wording of the reasons given for the motion deserves special notice: "In consideration of the great importance of Euler's ever-fresh works to the whole field of mathematical science, the German Association of Mathematicians hereby declares its readiness to actively support the publication of Euler's works as proposed by the Swiss Society of Natural Sciences and the association places at the disposal of the above-named society the sum of 5,000 francs to be taken from the funds of the association."

Further, in January last, the Paris Academy resolved to subscribe for 40 copies of the Euler edition (payable on receipt of each volume), on condition that it should appear in the original languages. This condition has been recently declared agreed to by the Swiss Euler Committee.

The entire cost of the Euler edition as planned has been estimated, after careful calculation and information received from competent firms, at 400,000 francs ($80,000), against which we may place at least 150,000 francs receipts from the sale of the books. If, as we confidently hope, the example of the Paris Academy is followed, and subscriptions come in in sufficient number, the financial effect would, of course, be more favorable.

We therefore now appeal to the mathematicians in every quarter of the globe and to all friends of the mathematical sciences for support and cooperation. We request them earnestly to procure for us as quickly as possible the subscriptions necessary for carrying out the enterprise, urging especially scientific libraries to become subscribers. The Euler edition will consist of about 40 volumes, and the price per volume will not exceed 25 francs ($5). The yearly expenditure will be thus comparatively insignificant, even if several volumes should appear in the course of the twelvemonth. And surely every mathematician will in future all the more insist on finding Euler's works in any library to which he has recourse, as at present these works are rarely to be met with, particularly in newer libraries, for example in America.

We further appeal to all the great mathematical associations to follow the example of the German Mathematical Association. At the same time, we beg their members to arrange the collection of voluntary contributions. As in Switzerland, so surely in other countries, the insurance companies, the more important technical societies, especially those of civil engineers, and such extensive industrial concerns as are based on mathematical-technical science, will be ready to cooperate for the success of our undertaking. For are

not Euler's works to be classed amongst the greatest of all ages, not only on the subject of pure mathematics, but also for their manifold technical applications!

We are hopeful that our appeal will meet with that interest on the part of all mathematicians which a complete edition of Euler's works may justly claim. So much preparatory work has already been done that it needs now but a comparatively slight effort from individuals and from scientific associations to insure the success of our great plan, the publication of Euler's works!

This appeal is signed by the presidents of the central committee and the Euler committee of the Swiss Society of Natural Sciences. Subscriptions may be sent to Professor F. Rudio, Dolderstrasse 111, Zurich V., Switzerland.

G. A. Miller

REPORT OF THE COMMITTEE TO VISIT THE MUSEUM OF COMPARATIVE ZOOLOGY

To the Board of Overseers of Harvard College: The committee wish to report that they visited the museum on April 29, and were received by Mr. Agassiz, the director, and by Mr. Henshaw, the curator, under whose auspices an inspection of the collections of the museum was made.

A walk through the museum can not fail to impress the observer with the careful forethought for great simplicity and security of construction, and the thoroughly scientific arrangement and handling of the vast collections therein contained. The architectural arrangement of the various departments is of the simplest character, and one can not but be impressed with the high degree of scientific accuracy displayed everywhere. The material on exhibition forms but a small part of the whole collection—more than two thirds of the specimens being stored for the purpose of scientific study. Among these are many great special collections—for instance, those of the Brazilian Expedition of Professor Louis Agassiz, and those of the numerous dredging expeditions of Mr. Alexander Agassiz, which have been the subject of classic memoirs.

Among the recent acquisitions of the museum which have already been described in previous reports, is the large model of the Bora-Bora coral reefs, which has been carefully prepared under the direction of Mr. Agassiz, and is now cased in the museum. It serves in its own way to commemorate the extensive and fruitful investigations into coral reef formation which he has made during recent years.

There are at present more than 45,000 volumes in the library, which, owing to its scientific value, has become one of Harvard's great possessions. It is eagerly sought out and consulted by students and masters in several departments of science, and not from Harvard alone, but from the whole United States and Canada. This great growth has made it necessary to place the books in stacks, and owing to this somewhat compact arrangement, the facilities for consulting the books are not so convenient as they would be if provision could be made for a more perfect system of artificial illumination.

With the growth of the museum, the need of a number of expert assistants in the preparation and mounting of specimens has been felt.

The first need to which we call attention would not involve great increase in the expenditures, but the second is a large item. As the resources of the museum and the director's private generosity are already taxed to the utmost, it rests with the university itself, or with the public, rather than with the museum, to bear this additional burden. Your committee feel strongly that the university should assume it.

It is appropriate on this occasion to call attention to the fact that this year marks the semi-centennial of the establishment of the Museum of Comparative Zoology. On April 2, 1859, the legislature of Massachusetts voted that the sum of $100,000 should be granted to the Museum of Comparative Zoology. In June, 1859, articles of agreement were made and executed between the trustees of the museum and the president and fellows of Harvard College. A piece of land about five acres in extent was deeded by the corporation to the museum, for the purpose of erecting a

fireproof building. The laying of the corner-stone took place with appropriate ceremonies on June 14, 1859. The formal establishment of the museum took place in October of the same year, by the presentation of Professor Agassiz's collections to the trustees.

The foundation was placed at first in the hands of a body of trustees appointed by the state, and it was not until 1875 that the museum was turned over and placed permanently in charge of the corporation of the university.

A small and lonely looking brick block which was erected at this time, with its four rooms to the floor, has grown nearly continuously around three sides of the great square originally laid out for it. A second section of the museum was soon added to the first, in 1871–2, and further additions were made in 1877, and again in 1880–2, and it was not until 1888–9 and 1901–2 that the last portions of the museum were finally added. The departments of botany and mineralogy and geology which have been added from time to time, and the Peabody Museum of Archeology and Ethnology—the building of which was begun in 1876—form portions of the present structure now known as the university museum, of which the Museum for Comparative Zoology forms the major part.

The university museum contains thirty-two rooms in its ground plan, and is by far the largest building of the university. Nor is it an empty shell—it barely serves to house its treasures and the students thereof.

A glance at the plans of this building shows what grand results have been accomplished in a half-century's work, and what has followed from the inspiration of a great teacher. Intellectually, the institution has grown from the daring experiment of a great enthusiast to an important position among the leaders of the world's museums and laboratories. It is doubtful if any department of the university has brought home to it from the old world more fame, or if any department has done more enduring work for time to come.

It is well to recall these historical memoirs on this occasion, and it must never be forgotten that such progress and development would have been impossible without the lifetime devotion of two very remarkable men—father and son. One is gone; in his life and death he has had great praise. He deserved it, and the university should ever commemorate him. One is with us; we should not wait until he is gone to give praise to him. He is a major benefactor to the university, and a great figure in her history. To him the university owes not only a lavish fortune spent in her service, but much more than this—the lifework of a great administrator, and a great scientific man.

Your committee, therefore, are unanimously of the opinion that this fiftieth anniversary of the Museum of Comparative Zoology should be fittingly celebrated by the whole university, and, further, that no celebration could be so fitting as one which would evince the university's admiration of the scholar and the man —Alexander Agassiz. It strongly recommends, therefore, to your board timely and positive steps in this direction, and tenders its services if desired for more concrete suggestions or consultation.

J. COLLINS WARREN,
Chairman

May 12, 1909

SCIENTIFIC NOTES AND NEWS

COLGATE UNIVERSITY has conferred its doctorate of laws on Dr. E. F. Nichols, president-elect of Dartmouth College. Dr. Nichols was professor of physics at Colgate University from 1892 to 1898.

WILLIAMS COLLEGE has conferred its doctorate of laws on Professor H. B. Fine, professor of mathematics at Princeton University.

THE degree of LL.D. has been conferred by the University of North Carolina on Dr. R. H. Whitehead, the new dean of the medical department of the University of Virginia.

ON the occasion of the Health Congress to be held at Leeds in July the honorary degree of LL.D. will be conferred by the university on the president of the congress, Col. T. W. Harding, and the honorary degree of D.Sc. on Sir

James Crichton-Browne and Major Ronald Ross.

The French Institute has awarded the Osiris prize to M. Louis Blériot and M. Gabriel Voisin, the aeronauts and engineers, in recognition of their achievements and experiments in aerial navigation. The prize, which is of the value of £4,000, is awarded every three years to the person or persons who during that period shall have made the most remarkable contribution to the cause of human progress.

The Pharmaceutical Society has awarded the Hanbury gold medal to Professor W. O. A. Tschirch, professor of pharmacognosy and practical chemistry at Berne University.

Sir Felix Semon, K.C.V.O., physician extraordinary to the king, is about to retire from practise, and a complimentary banquet is to be given him by his professional and other friends on July 2. The organizers of the banquet are anxious to found a lectureship or scholarship in his name to be a record of his scientific work.

We learn from the *Journal* of the American Medical Association that Professor A. Tamassia, of the chair of medical jurisprudence at the University of Padua was recently elected member of the Italian senate, and his election and the thirtieth anniversary of his connection with the university were celebrated simultaneously on May 29 by the presentation from the citizens, faculty and students of parchment testimonials, a gold medal, a bust of himself and a banquet.

Dr. Gottfried Galle, the eminent astronomer, who since his retirement from Breslau twelve years ago, has been living in Potsdam, has celebrated his ninety-seventh birthday.

Dr. Edward Pflüger, the eminent physiologist, has been made an honorary citizen of Bonn, on the occasion of his eightieth birthday.

Dr. Samuel C. Chew, professor of medicine in the University of Maryland, has resigned his position, which he has held for nearly forty-five years.

Professor Lloyd Tanner, M.A., D.Sc., F.R.S., has resigned the chair of mathematics in Cardiff University College, after twenty-six years' service.

The German Physiological Society has been meeting at Würzburg, under the presidency of Professor Max von Frey.

Frederic Walton Carpenter, Ph.D., has been appointed by the president and fellows of Harvard College to be director of the Bermuda Biological Station for Research for the summer session of 1909.

Dr. Ralph Arnold, geologist, U. S. Geological Survey, formerly of Washington, D. C., resigned on June 1 and moved to Los Angeles, California, where he expects to continue his scientific investigations in connection with professional work as a consulting geologist and engineer.

Mr. H. Helm Clayton has resigned as meteorologist of the Blue Hill Meteorological Observatory, with which he has been connected for many years. Mr. Andrew H. Palmer, a graduate student of meteorology at Harvard University, has joined Professor Rotch's staff.

Mr. Pierce Larkin, A.B., University of Oklahoma, has been elected to a fellowship in the department of paleontology in the University of Chicago. He will spend the summer in collecting vertebrates in the Oklahoma Redbeds.

Messrs. U. S. Grant and D. F. Higgins sailed from Seattle on June 8 for Seward, Alaska, where they will begin a reconnaissance of the eastern part of Kenai Peninsula. They will use a large launch for traveling, as the work will necessitate passing many headlands that are open to the full sweep of the Pacific.

Professor W. E. Stimpson, of the University of Kansas, has received a year's absence to work in the Bureau of Standards at Washington.

Professor Charles Edward A. Winslow, of the biological department of the Massachusetts Institute of Technology, has been granted leave of absence for three months, so that he may take the place of Professor E. O. Jordan, of the University of Chicago, during this fall when he will be in Europe.

DR. SANTIAGO ROTH, of the La Plata Museum, Argentina, has accepted membership on the International Correlation Committee of the National Academy of Sciences. The object of this committee is to obtain data for a better correlation of the geological succession in different parts of the world, a matter in which there is at present wide diversity of opinion. Dr. Roth will contribute especially upon the Mesozoic and Cenozoic formations of the Argentine Republic.

AT the commencement exercises of the medical department of the University of North Carolina, Dr. William H. Welch, of the Johns Hopkins Medical School, delivered the address. Dr. Welch has now sailed for Europe.

A STATUE in honor of Theodor Swann, the physiologist, has been erected at Neusz where he was born.

PROFESSOR CARL N. I. BÖRGEN, for thirty-four years director of the Imperial Observatory at Wilhelmshaven, has died at the age of sixty-six.

DR. MARIA ARISTIDES BREZINA, formerly director of the mineralogical department of the Imperial Vienna Museum, died in Vienna on May 25, after a long illness, in the sixty-second year of his age. Dr. Brezina was one of the most brilliant and thorough of the European mineralogists. He devoted the latter part of his life to the study of meteorites and he created a vast amount of important literature on the subject, incidentally making the meteoric collection in the Imperial Museum in Vienna the most complete in existence. The mineralogical collections prospered under his direction; indeed, the Viennese collections were for many years the leading ones in Europe. Dr. Brezina leaves a wife, two daughters and a sister.

UNIVERSITY AND EDUCATIONAL NEWS

THE mechanical and electrical engineering laboratories of the Rensselaer Polytechnic Institute, erected at a cost of about $400,000 from the fund given by Mrs. Russell Sage, were dedicated on June 15, at the eighty-fifth commencement of the institution. Addresses were made by Jesse M. Smith, president of the American Society of Mechanical Engineers and by Louis B. Stillwell, president-elect of the American Institute of Electrical Engineers.

FOR the purpose of maintaining at Cornell University a number of students who are to pursue research work in engineering, Mr. L. L. Nunn, of Telluride, Colorado, is building on the campus and will endow a clubhouse in which the investigators that he sends there are to live.

A FELLOWSHIP in chemistry of the annual value of $500 has been endowed by Dr. Milton L. Hershey, of Montreal, in the School of Mining, Kingston, Ontario. It is open to graduates of all universities and technical colleges.

MR. CHARLES MARTEN POWELL, formerly scholar of Corpus Christi College, Oxford, has given £250 a year to the college in order to enable it to augment the stipend of White's professor of moral philosophy, in accordance with the statutes of the last commission.

KING EDWARD will lay the foundation stone of the first new building of the Imperial College of Science and Technology for the departments of mining, metallurgy and geology (Royal School of Mines), and in extension of the engineering department (City and Guilds College), on July 8. The buildings are to be erected on the land in Prince Consort-road, South Kensington, lying to the east of the Royal College of Music and to the north of the City and Guilds College.

AT Harvard University Edward Murray East, of the Connecticut Agricultural Experiment Station, has been appointed assistant professor of experimental plant morphology, O. K. O. Folin becomes Hamilton Kuhn professor of biological chemistry, E. E. Southard, Bullard professor of neuropathology and Myles Standish, Williams professor of ophthalmology.

AMONG the promotions to full professorships at the University of Wisconsin are Dr. Richard Fischer, analytical chemistry, and Dr. H. C. Taylor, agricultural economics. The following were promoted from assistant profes-

sorships to associate professorships: Eliot Blackwelder, geology; C. M. Jansky, electrical engineering, and E. G. Hastings, bacteriology. Those who were raised from instructors to assistant professors include George Wagner, zoology; E. R. Jones, soils; C. P. Norgord, agronomy; M. O. Withey, mechanics; W. S. Kinney, structural engineering, and A. G. Christie, steam engineering.

At Princeton University Dr. E. P. Adams, assistant professor of physics, has been elected professor of physics and Dr. L. P. Eisenhart, preceptor in mathematics, professor of mathematics. Among the preceptors elected were: P. H. Fogel in philosophy; J. G. Hun, C. R. Maclunes and Elijah Swift, in mathematics. The following instructors were appointed: E. B. Baxter, in philosophy; B. J. Spence, in physics, and C. M. Dennis, in civil engineering.

In the Johns Hopkins Medical School Dr. Charles D. Snyder, has been appointed associate in physiology. Among the instructors appointed are: Dr. Leonard G. Rowntree, in experimental therapeutics; Dr. Arthur H. Koelker, in physiologic chemistry; Dr. Herbert M. Evans, in anatomy, and Dr. Milton C. Winternitz, in pathology.

Dr. John C. Shedd, of Westminister College, Denver, has been elected professor of physics and head of the department at the University of Pittsburgh. The department will move into the new laboratories now nearing completion. Mr. Will Grant Chambers, for the past five years professor of psychology in the Colorado State Normal School, has been elected to the newly established chair of education in the same institution.

Dr. C. H. Shattuck, associate professor of botany and forestry at Clemson College, S. C., has recently resigned to accept a professorship in the University of Idaho.

Mr. H. P. Kean, assistant in the University of Illinois, has been elected professor of mathematics in Ripon College.

At the University of Cambridge Mr. H. F. Newall, F.R.S., fellow of Trinity College, has been elected to the recently founded professorship of astrophysics.

DISCUSSION AND CORRESPONDENCE

JOINT MEETINGS OF ZOOLOGICAL SOCIETIES

To the Editor of Science: There has been brought to my attention much severe criticism of the fact that at the recent Baltimore meeting Section F held meetings parallel with those of the American Society of Zoologists; and many of these criticisms are in such a form as to suggest that the officers of Section F deliberately planned that conflicting arrangement. Such is not the case. The officers then responsible for Section F were also members of the American Society of Zoologists and therefore were especially anxious to avoid conflicting meetings. For some weeks preceding the meeting, the secretaries of the American Society of Zoologists and Section F carried on correspondence looking towards a joint program, with the exception of the vice-presidential address before Section F. In the last days before publication of programs, it suddenly became evident that the large number of papers submitted made a joint two-day program as planned impossible, and there was no time for considering possible rearrangements of plan.

The problems of conflicting meetings have now been more carefully considered by the sectional committee of Section F; and with the hope of leading to better organized zoological meetings, with grouping of papers into natural subdivisions and perhaps sectional meetings when necessary in order to complete the program of papers in two or three days, the committee has voted that if the American Society of Zoologists goes to Boston next December Section F will propose limiting its meetings to the vice-presidential address and business, and leave to the American Society of Zoologists entire charge of the meetings for the reading of zoological papers. Such a proposition has been transmitted to the secretary of the American Society of Zoologists for reference to the executive committee of that society.

The writer has heard only one line of objection to the general plan of this proposition, namely, that Section F has among its members several hundred who are not eligible to

membership in the American Society of Zoologists. This is true and is one of the strongest reasons for the existence of Section F as an organization independent of the American Society of Zoologists. However, so far as experience at meetings in recent years goes, this large membership of Section F has no important bearing on the question of a consolidated program of papers under the auspices of the American Society of Zoologists. There have probably not been a half dozen papers read before Section F in the past three years by authors who were not also members of the American Society of Zoologists or who could not have obtained an introduction to that society for the reading of their papers. Moreover, the sectional committee of Section F has constitutional authority for rejecting papers not satisfactory in preliminary abstracts; and since the members of that committee are also members of the American Society of Zoologists there is no reason to suppose that an irresponsible member of Section F could get an opportunity to read a paper in a consolidated program with the American Society of Zoologists.

A second objection is that the large audiences composed of members of Section F inhibits discussion and as a result zoological meetings are not so helpful as they were years ago. This is absurd to one who observed the record-breaking run of papers made by the American Society of Zoologists at Baltimore on the days when Section F held its own meetings. It is evident that the American Society of Zoologists has already overgrown in scope, in membership and in productivity of members; and soon must consider some natural subdivision in order to gain the time for the deliberate work which was once so satisfactory.

It is true, as charged by certain members of the American Society of Zoologists, that the majority of papers read before Section F are by the younger group of zoologists. But may not these men just entering the zoological field have some right to the inspiration and criticism derivable from reading papers before a body of older zoologists? Have those who object to the reading of papers by the younger men forgotten that ten or twenty years ago they too were just emerging from the graduate schools and were eager to present their research work? The need of an opportunity for those not yet eligible to membership in the American Society of Zoologists is alone sufficient justification for regular programs of Section F whenever the American Society of Zoologists does not adopt some such grouping of papers and parallel sectional meetings as will permit the reading of all zoological papers worthy of serious consideration. If the officers of the American Society of Zoologists are willing to make such an arrangement, the present officers of Section F will cooperate fully in the selection of papers offered by members of Section F who are not also members of the American Society of Zoologists, and after that will leave the programs for reading of papers entirely under the auspices of the officers of the American Society of Zoologists. But if such a consolidation is not acceptable to the American Society of Zoologists, the officers of Section F will continue to consider it their duty to arrange otherwise for the reading of worthy papers by men who do not have an opportunity to present results of their research before the American Society of Zoologists.

MAURICE A. BIGELOW,
Secretary of Section F

TEACHERS COLLEGE,
COLUMBIA UNIVERSITY

THE BIRTHPLACES OF LEADING AMERICANS AND THE QUESTION OF HEREDITY

IN SCIENCE, April 9, I challenged the following statement of Mr. W. J. Spillman:

> With only 29 per cent. of our population actually living on the farm, with miserably poor school facilities as compared with our city population, this 29 per cent. furnishes about 70 per cent. of the leaders in every phase of activity in this country.

In SCIENCE, May 7, Mr. Spillman corrects his 29 per cent. to about 36 per cent. He admits that he has "no way of ascertaining how many of the men who are distinctly leaders in this country were actually brought up

on the farm." He also says he merely repeated a statement he had heard frequently and which he had never heard challenged.

Mr. Spillman, however, doubts the significance of the facts which I presented in SCIENCE, April 9, drawn from "Who's Who in America" showing that centers of population of 8,000 and over have produced about twice their expected ratio of persons included in this volume. I do not myself claim that these figures give a final refutation to the belief which Mr. Spillman holds, that there is a special value to be assigned to life on a farm during early boyhood, but there appears to be certainly no evidence at present to support such a view. Future investigation may show that the farms give a higher ratio than the small towns and villages, but the inference from such data as we have is in favor of concentrated centers of population against sparsely settled regions in general. This idea must stand until special researches show that it does not apply to towns and villages against farms. In any event, the actual farms have probably produced much less than 70 per cent. of the total leaders of the present day, since 30 per cent. are found from the cities alone, and this would leave nothing for the towns and villages.

Mr. Spillman's second letter contains so much that requires discussion or correction that I am forced to quote nearly the entire article, taking the points up one at a time.

Although I thank him for his complimentary references to my study of heredity in royalty, still I am sorry that he has introduced this more complicated discussion just here. But since in the first part of his article he has done so, I would like to correct one or two misinterpretations of my position on the question of environment *versus* heredity. Mr. Spillman says:

> I have only one criticism of Dr. Woods's reasoning. In studying heredity in royalty he purposely chose this class because it could be assumed that their characters were formed under the most uniform environment, which purpose was of course entirely legitimate.

This was really not my purpose in choosing royalty as a basis of study; and, furthermore, I do not think that their characters were formed under a uniform environment. I have found in studying their lives the greatest variation in their environments, all the way from the supposed advantages of a wholesome family life, with famous tutors to give them learning, or the call of warfare to grant opportunity, down to the foul atmosphere of some Bourbon court, or the mouldering walls of a prison cell.

On all this I have dwelt in the same book which Mr. Spillman here alludes to.[1] On page 9 I say:

> Although all have the highest social rank, they have lived in different countries, in different centuries, and under varying circumstances, with different educations and opportunities.

The same theme is expressed more fully on page 284, and occurs here and there throughout the whole work.

Mr. Spillman goes on to say:

> But it must be remembered that this [royal] environment is the best possible for the development of character and ability.

I do not see how Mr. Spillman can feel justified in making a positive assertion on this point, so many diverse opinions are held. Besides, it is begging the issue which I raise, that no one has shown that any ordinary civilized environment is more influential than any other in molding the rough outlines of character or determining the end product of achievement. I should say that we have no means of knowing whether the royal environment is on the whole favorable or unfavorable. Some investigators like Jacoby and Galippe, along with journalistic writers, have assumed it to be very unfavorable, though without any satisfactory proof.

Mr. Spillman adds:

> It would be gratifying to me to see Dr. Woods make a similar study of some class of human beings subjected to an unfavorable environment. I believe he would find, as I have stated above, that even in that class native ability and natural impulses would prove to be purely a matter of

[1] "Mental and Moral Heredity in Royalty," New York, Holt, 1906.

heredity; but that character and actual ability would be found to be profoundly modified by environment. In fact, the whole experience of the human race speaks for this assumption. If the opposite were true, then why should the state go to the expense of maintaining schools, for a man's effectiveness would not depend on his environment but upon his inheritance.

I am glad to answer this point because I think that some of the confusion which ordinarily accompanies the discussion of this time-honored question may be lessened; and I should like at the same time to state my own position in the matter. I have never claimed that great alterations in the environment of man would not produce any results.

It is, at the outset, both necessary and easy to recognize that there are in general two ways that a man may stand in relation to his environment. First, it may be an environment from which he can not escape, try as hard as he may; and second, it may be one from which he can escape, if the inherent tendencies are strong enough. These two arbitrary classes may shade into each other at times; but I think it will aid in clearing up some of the usual perplexity which clings about the subject, if I am permitted to formulate these two categories. An accident which affects the brain or sight, a long term of imprisonment, or confinement to a desert island, may serve as examples of environments from which there is no escape. In a like way the same may be said of the epoch, or period of civilization in which a man's life falls. All such environments must *a priori* modify character more than conditions from which there is a ready, or even possible, escape should the innate impulses crave it. Now the complete cessation of all schools of education would be establishing an environment from which there would be no escape. Except that there would still be libraries, it would be like returning to the educational possibilities at the time of the Teutonic tribes. It would be suddenly changing one period of civilization into another. This would of course work a profound detriment to all mankind. But there would still be marked individual differences of achievement.

Now the point is that in any given age, or in any one civilization, there are always these marked differences of achievement or moral character. The question then arises—Are the differences in environment which have ordinarily existed in the past, within any one age, or do exist at the present time, of sufficient magnitude or force to cause evident or measurable differences among men?

At least within modern centuries, and since the days of serfdom, it is possible for a man to pass from one set of surroundings into another if the inborn desires and abilities are strong enough. Thus here we have a condition coming under the head of an environment of the second class or one from which escape is possible, and therefore we do not expect to find environment working at its maximum, as we do for instance in experimental zoology where the conditions are imposed and unescapable. But more important than this general argument is the fact that no one has shown that such variations in surroundings as occur in the average lives of human beings (riches or poverty, good or bad education, etc.) are in any way responsible for the rougher differences found among men. I say rougher differences because it is only into rough grades and scales of difference that psychic qualities have so far been classified by the few investigators who have worked upon such problems. I suppose that even the ordinary variations in circumstances which befall mankind produce some change in character and achievement. I do not know. All I say is that no one has succeeded in demonstrating it. I myself searched for it in the statistics of royalty by five different methods, but failed to find it there. I concluded that the force of environment is in general slight, in accounting for mental and moral differences, perhaps measurable when more delicate methods should be devised. Galton, from a study of twins,[2] places "nature" over "nurture," though only vaguely so; but Thorndike, in his "Measurements of Twins,"[3] goes fur-

[2] "Inquiries into Human Faculty," 1883.

[3] *Arch. of Philosophy, Psychology and Scientific Methods*, No. 1, September, 1905.

ther than Galton, and precisely confirms my own estimate, made in 1902, of about nine tenths for heredity. Barrington and Pearson have recently found that the influence of environment on sight is *nil*.[1] Thus the prediction is already strongly in favor of future investigators arriving at a similar result, that the ordinary influence of environment on the higher human attributes is at most but trifling when the heredity factor remains the same, or when the heredity factor can be measured, or eliminated from the discussion.

Usually it is not possible to separate heredity from environment. We often merely find some correlation which may be explained as due to either or both of these forces. An example of this sort of correlation is the one I found to exist between general superior achievement in the United States and city birth. I have already explained in my other article why some correlation is to be expected from inheritance alone, while from environment it may or may not be expected. No one is in a position to speak on the latter question because no one knows whether, on the whole, the good and bad sides of city life strike a balance in favor of the city against the good and bad sides of country life. And even if the answer to this complicated problem were known, we should not then know if there were anything efficacious enough to produce a measurable result, as I have explained above.

The failure to find a higher ratio for the cities would have been a serious blow for heredity. The finding of a higher ratio for cities merely wards off a possible attack. It is a purely negative defense, and this is all I have claimed for it. Now, in his second letter Mr. Spillman positively asserts: "Dr. Woods's own figures prove the effect of environment as against heredity." In the light of the explanation in my former letter, which I have here just now, in other words, repeated, I ask, How can he possibly know this? How can he know that the figures are even beyond the expectation from the heredity factor alone?

Mr. Spillman then complains that the persons listed in "Who's Who in America" do not represent leaders. I do not wish to enter into a discussion on the use of words, and for the sake of the argument will grant that they shall be called merely "competent workers," but I can not refrain from saying in passing that "leaders" seems a very fair word to apply to a group so small as 16,000 out of a total population of some 80,000,000. I should think of the colonel of a regiment of a thousand as the leader of that thousand and it would be in no less just a way and with as good a proportionate sense to call the higher railroad officials, greater bankers and relatively few doctors, lawyers, etc., whose names are included in this same volume "leaders" in their special fields of activity. But even if we are to call them mere "competent workers," are not just such "competent workers" to be desired, whatever be the cause of their competency?

I knew that "Who's Who" itself would be criticized, therefore I forestalled this criticism by the following:

> Some will not be willing to accept conclusions drawn from a list which like this doubtless has certain flagrant omissions, and where he sees names that he considers should not have been included. If he will stop for a moment and think, he will see that the very objection he raises only argues in the other direction from what he supposes. If, for instance, I find a marked correlation between city birth and more or less notable subsequent achievement, drawn from an imperfect list, the correlation would be even higher were the list of names ideally perfect.

Mr. Spillman makes no reference to this. The same applies whether the list be lower in standard than it should be owing to errors within itself, or whether it be in general a list showing a low standard of selection. Providing of course that the standard be above the general average of the population, then it follows that the higher the standard the higher would be the correlation, at least as far as mathematical expectation is concerned.

Mr. Spillman then takes a prop from the

[1] "A First Study of the Inheritance of Vision and the Relative Influence of Heredity and Environment on Sight," *Eugenics Laboratory Memoirs*, V., 1909.

25 presidents of the United States "23 of whom were country bred, or were brought up under what the census terms rural conditions." This fact is without significance for two reasons. First, the total 25 is so small that the probable error is necessarily too large to give a conclusion in a statistical discussion of this kind. Second, and equally important, their birth records must be taken in terms of the total proportion of the population dwelling in the country and rural districts in the early days when these men were born. The fact that "about 36 per cent. of our population actually live on the farm at the present time" has nothing to do with the question. The same criticism applies to the figures concerning United States senators. He has shown no ratio over the expected for the rural regions in terms of population distribution, at the time of their birth, some fifty or sixty years ago.

I shall look forward with expectancy to the other statistics which Mr. Spillman hopes to present, and am very glad that he takes an interest in these questions. I agree with him that "the matter must rest here until further statistics are available"; but in the meantime I shall feel much confidence in the indications which have been furnished me as drawn from a list of some sixteen thousand, more or less notable persons, out of the vast population of the United States.

FREDERICK ADAMS WOODS
BROOKLINE, MASS.,
May 17, 1909

FAIR PLAY AND TOLERATION IN CRITICISM

To that large number who accept the justice, the value and the need of the recent criticism by Blackwelder of the geological fallacies dressed out as facts in Lowell's book on Mars as the abode of life, some reply will seem called for to offset before the general scientific public the personal, befogging and dogmatic rejoinder which it evoked in a recent issue of SCIENCE from one not a geologist.[1] In this connection some preliminary statement may well be made as to the kind of articles which in the mind of the writer seem to call for certain kinds of criticism. This appears the more necessary since to some all criticism seems out of place and to indicate a carping disposition, while others would hold that specialists are too lax in permitting to pass unchallenged many works which are highly erroneous but whose character is evident to the specialist only.

Destructive criticism is to all constructive workers in science a disagreeable task, yet one which should often be regarded as a duty, especially to university teachers, since such are deeply interested in the general diffusion of knowledge and should be equally concerned in the prevention of that diffusion of error which, unless vigorously combated, takes the place of truth.

All research work, even by properly qualified men, must necessarily contain some percentage of error which is eliminated by further advances in knowledge, but which frequently serves a most valuable purpose in stimulating to further and more exact observation and analysis. Such work, addressed to specialists, is always worthy of more praise than criticism, and a proper review will always seek out the parts of value and give them more prominence than those features which in the mind of the reviewer may seem open to question or even to miss the truth. It is not such research work which is here under discussion.

Advancement of knowledge, however, implies not only abstruse technical researches, but popular expositions of the same which shall carry a vivid conception of the principles and results to the intelligent but unprofessional public, consisting of laymen as well as workers in other branches of knowledge. Such work when well done is regarded by scientists in general as of the very highest educational value, and many eminent men have contributed a part of their time to the development of popular science. In fact, no small part of the eminence of some of the best known and highly regarded men of science is due to their work in what may be called the popular field, since it reaches those whose professional interests are in other branches. It is obvious

[1] "Fair Play and Toleration in Science," by T. J. J. See, professor of mathematics, U. S. Navy, SCIENCE, Vol. XXIX., pp. 858–60, May 28, 1909.

that it is not against work of such character that Blackwelder's review is directed.

Again, there is a large class of fugitive popular scientific literature written by men of no personal reputation, bearing within it the marks of its unauthoritative nature, some of it good, some bad. Such articles hardly call for serious comment from specialists.

But there are popular works ably written and put forth in a garb of authority which, however, confuse facts, theories and hypotheses, and contain views regarded by the great body of those qualified by special knowledge to hold an opinion as outworn, or wholly erroneous and misleading. It is against such false science, not popular science, that public and severe censure becomes a duty. As Blackwelder admirably puts it, unless such criticism is directed against such a book and its author "the average reader naturally believes him, since he can not without special knowledge discern the fallacies. He has a right to think that things asserted as established facts are true, and that things other than facts will be stated with appropriate reservation. This is precisely the same as his right to believe that the maple syrup he buys under that label is not glucose, but is genuine. The misbranding of intellectual products is just as immoral as the misbranding of the products of manufacture."

This code of morality makes it the duty of the teacher and scientist to expose in print such scientific shams, a duty, however, which is always disagreeable and which the majority of men leave to their fellows to do. He whose time is fully occupied with teaching and research, but who turns aside to do the task which others have left undone, is therefore deserving of honor and not of abuse.

It is noteworthy that Lowell's book on "Mars as the Abode of Life," in spite of its mass of fundamental errors whenever geological matters are touched upon, errors palpable to every working geologist, has been before the public for more than a year without any criticism of these features appearing in Science, the official organ of the American Association for the Advancement of Science, an association which since the development of special societies has become devoted to the general broadening of scientific knowledge. Such a criticism seems especially called for, since the book has been given the very widest publicity, it deals with a subject of great popular interest, and its author has been grandiloquently advertised by his publishers as the "founder of the new science of planetology." As an illustration of the result it may be noted that in the scientific columns of a carefully edited popular weekly its author has been hailed as one who would henceforth relieve America from the European taunt that it had as yet produced no really great and creative man of science.

As an offset, however, to the necessarily severe criticism of "Mars as the Abode of Life," cordial recognition may well be given at the same time to that great enthusiasm manifest in all of Lowell's work, which has led to the founding of a magnificent observatory and has contributed to astronomy much of real value. A coming generation of scientists will find much to regard highly in Lowell and will see in his work a stimulus to further knowledge, but will hold it as unfortunate that the same temperament which led to these results should have given rise to writings which called forth such severe criticisms as have appeared from his contemporaries in order to separate errors of premise and conclusion from that which is of real value.

Having made these preliminary statements, the true character of See's arraignment of Blackwelder may be shown by calling attention to the several topics which are discussed.

Blackwelder's review is aimed at false science, not against popular science, regarding which he says not a word; yet See uses a column and a half to flay him on that score, and because Blackwelder criticizes Lowell assumes that the criticism is aimed also against the popular work of such men as George Darwin and Proctor.

Blackwelder specifically avoids discussing any astronomic phase of the book, and does not mention the subject of life on Mars. Yet See takes up a column in arguing this matter,

and states: "Of course there is life on Mars; there is no doubt about it."

Lowell has been fortunate in being able to personally build and maintain an observatory, which has been the means of advancing the science of astronomy in a number of lines. See asks what Blackwelder has done in comparison. This question implies that only those whose personal fortunes have enabled them to do what Lowell has done should criticize his work, since those familiar with the scientific results of both will hardly see cause on such lines for invidious comparison.

Blackwelder casually mentions, to the extent of one sentence, "Lowell's implicit belief in the Laplacian hypothesis which now, to say the least, is on the defensive," a remark which calls forth a column from See embracing such statements as, "If Professor Blackwelder will study my own (See's) paper carefully, and the work now in press (by See) when it appears, he will find that most of the recent speculations on cosmogony are not worth the paper they are written on."

See further states that he has proved in four memoirs "that the oceans are gradually drying up and the land increasing, as Lowell maintains. Therefore Lowell is right and Blackwelder wrong; and that too in a subject which he represents as his own." This statement is highly amusing, to say the least, to those cognizant of recent work on paleogeography, especially if they have also read See's voluminous publications on mountain building and related subjects, and noted that they center about the old hypothesis of a free downward permeation of ocean water. A hypothesis which is not open to direct proof, and though still advocated by certain physicists and geologists is distinctly relegated to a subordinate rôle by many economic geologists and such leaders in the more philosophic side of the earth-science as Suess, Chamberlin and Van Hise; partly because of the theoretical difficulties attending an effective downward diffusion of ocean water through the zone of rock flowage, but much more because of the failure of the hypothesis to account for many of the facts now known to geologists. These point rather to a directly opposite view, which is well expressed by the words of Suess, "volcanoes are not fed by infiltration from the sea, but the waters of the sea are increased by every eruption."

The voluminous nature of See's writings on the subject is due to a dressing out of this old and, to say the least, doubtful hypothesis with many speculative additions, with much repetition of well-known facts and theories, and with specific applications in such frequent obvious discord with modern teaching of the principles of physiography and known details of geologic structure and history, that no geologist has felt called upon to comment. In the words of See, "geologists have discreetly kept silent."

On every topic See cites his own work as the authoritative utterances on the subject, and in the last paragraph denounces, as the worst evil of American science, "this clique and faction business, by which a man who is not in the ring never can get justice or fair consideration." Since no group of geologists or, so far as the writer is aware, no single geologist of recognized standing has followed and promulgated the special views in the teachings of See and Lowell, this clique and faction evidently includes the several hundred working geologists of America. To those who are familiar with the situation, this gives the key to the whole of See's article on "Fair Play and Toleration in Science." It is a vicarious castigation in which Blackwelder stands to receive the blows for a host of unnamed men of science, because they have not accepted See's memoirs at the valuation which he places upon them. Is vicarious atonement "fair play and toleration in science"?

JOSEPH BARRELL

NEW HAVEN, CONN.,
June 15, 1909

DETERMINATION OF THE COEFFICIENT OF CORRELATION

TO THE EDITOR OF SCIENCE: I should like to make a few remarks on Dr. Franz Boas's letter on this subject in your issue of May 21. There is some danger, I think, unless we see

how new values for the correlation coefficient are related to old values, in a multitude of formulæ leading to divergent and possibly inconsistent results.

Dr. Boas's first value for r

$$-r=\frac{[(x-y)^2]-\sigma_x^2-\sigma_y^2}{2\sigma_x\sigma_y}$$

is a very old friend indeed and has been widely used in a multiplicity of practical cases. It is one of a general series of formulæ noted by me[1] in 1896, and used in our work on the personal equation[2] in 1902 and on wasps[3] in 1906. Since 1896 it has been frequently referred to, *e. g.*, in the memoir "On Further Methods of Determining Correlation"[4] and *Biometrika*, VI., p. 438, etc. It is quite reliable and often convenient.

Dr. Boas's second formula

$$r=\frac{[(x_1+x_2+\cdots+x_n)]^2}{n(n-1)\sigma_x^2}-\frac{1}{n-1}$$

suffers from the difficulty that in the form in which he gives it, it involves the number in the fraternity, being taken as *constant*, whereas in practise we may often have five in one fraternity and ten in a second. Its chief value is when n is very large as in long series of homotypic characters, or in series other than man when the number of offspring is very great. In such cases the second term $1/(n-1)$ is usually of the order of our probable error and may be neglected and $n-1$ may be taken $=n$, within the same limits. Thus:

$$r=\frac{(\text{S.D. of means of fraternities})^2}{(\text{S.D. of population})^2}.$$

Under this aspect it is easy to extend the formula to cases in which n is not the same for each fraternity. A like formula was used in 1898 for our studies on the inheritance of fecundity of thoroughbred horses.[5] It has been since employed in various homotypic investigations. It must be very carefully distinguished from that for the correlation rates

$$\eta=\frac{\text{S.D. of means of arrays}}{\text{S.D. of population}}$$

where $\eta=r$ for normal correlation.

The *arrays* in the latter formula contain many *fraternities*, and their means have far less variability than that of those of fraternities.

Lastly I come to Dr. Boas's formula

$$r=\frac{p_{1,2}-p_1p_2}{\sqrt{p_1(1-p_1)p_2(1-p_2)}}.$$

If we have a fourfold table represented by

a	b	$a+b$
c	d	$c+d$
$a+c$	$b+d$	N

I find Dr. Boas's r is our old friend

$$r_{bk}=\frac{ad-bc}{\sqrt{(a+b)(c+d)(a+c)(b+d)}},$$

i. e., is the correlation in the deviation of the mean of one variable from its mean value with the deviation of the mean of the second variable from its mean value. It is not a true correlation of the first variable with the second variable. I have discussed v_{bk} at length in memoir of 1900:[6]

> It has the advantage of a symmetrical form and a concise physical meaning. It does not, however, become unity when either, but not both b and c vanish, nor does it, unless we multiply it by $\pi/2$ and take its sine, equal the coefficient of correlation when $a=d$ and $b=c$.

Thus it differs in the simplest cases from the true coefficient of correlation, and often differs considerably. In the bulk of cases it does not approach r nearly as closely as the "Q_s" coefficient of association, and its use is liable to be misleading, especially if compared with values of the true coefficient found by other processes.

When there is a measurable quantity grouped in arrays under classes of a non-measurable quantity the right method, I venture to think, is to use the correlation ratio η as defined above. This will be equal to r if the correlation be normal, and if not it has a perfectly definite physical meaning of its own.[7]

[1] *Phil. Trans.*, Vol. 187 A, p. 279.
[2] *Phil. Trans.*, Vol. 198 A, p. 243.
[3] *Biometrika*, Vol. V., p. 409.
[4] Dulan & Co., Drapers's Research Memoirs.
[5] *Phil. Trans.*, Vol. 192 A, p. 272.
[6] *Phil. Trans.*, Vol. 195, pp. 12 and 15 bottom.
[7] "On the General Theory of Skew Correlation, etc.," Drapers's Research Memoirs, p. 10.

I am not able to follow Dr. Boas's deduction of a formula for r in this case, and it does not appear to give the true correlation r of the two variables.

KARL PEARSON

BIOMETRIC LABORATORY,
UNIVERSITY COLLEGE, LONDON,
June 6, 1909

THE DARWIN CELEBRATION AT CAMBRIDGE

TO THE EDITOR OF SCIENCE: I shall be obliged if you will allow me to contradict a statement which has been made in an American newspaper in reference to Professor Haeckel and the Darwin Celebration. The article in question was sent to me by a friend as a cutting and I am unable to give the name of the newspaper. The writer of an article entitled "Haeckel, the fighting scientist retires from Jena University," says: "He (Professor Haeckel) would have been glad to accept an invitation to the Cambridge celebration of the Darwin centenary—had he received it. None came, however, although a large number of such invitations have been sent to scientists who, to say the least, are no more distinguished than himself and to hundreds of scientific societies. It is strongly suspected that clerical prejudice has had a large share in this extraordinary omission. It is quite unjustifiable, for, whatever may be thought of Professor Haeckel's philosophic speculations, not even his enemies venture to deny his great service in the development of Darwinism."

The facts are these: A large number of universities, academies and learned societies were invited by the University of Cambridge to appoint delegates to attend the Darwin Celebration in June of this year. In response to this invitation the University of Jena appointed Professor Haeckel as its delegate. At a later date, after replies had been received from universities and other corporate bodies, several invitations were sent to individuals other than those already nominated as delegates. A short time ago Profesor Haeckel wrote to express his regret that ill-health rendered a visit to Cambridge impossible, and his successor in the chair of zoology, Professor Plate, was appointed in his stead. I need hardly add that if Professor Haeckel had not been appointed a delegate he would certainly have been invited as a private guest. I may state that some years since Professor Haeckel received from Cambridge University the honorary degree of doctor of science.

I am, Yours faithfully,

A. C. SEWARD

One of the Honorary Secretaries to the Darwin Celebration Committee; Professor of Botany in the University.

BOTANY SCHOOL,
CAMBRIDGE, ENGLAND

QUOTATIONS

VIVISECTION

LITERARY reference or allusion makes readable sometimes the barer facts of science. The vogue of Rudyard Kipling will render more popular a scientific cause to which he happens to lend his name. It is for that reason, rather than for the value of his statement, that we quote the poet as follows on a question of the day:

> The doctor is exposed to the criticism of persons who consider their own undisciplined emotions more important than mankind's most bitter agonies; who would cripple and limit research for fear research might be accompanied by a little pain and suffering. But if the doctor has the time to study the history of his own profession he will find that such persons have always been against him—ever since the Egyptians erected statues to cats and dogs on the banks of the Nile.

The opponents of vivisection ought to oppose murder, and therefore to be vegetarians. They should also object to forced labor and therefore never ride behind a horse. They should in sound logic oppose larceny and not drink milk. They should never allow an animal to be punished in process of being trained. In scientific experiment few animals are taken, compared to those killed for food or kept at forced labor all their lives. Most of them are unconscious. The question of when to use anesthetics must be left to science, since in a small but important fraction of the work drugs must be dispensed with; and it would be fatal to have ignorant outsiders concerned in so critical a decision. Such outsiders are cap-

able of judging sanely neither about the amount of pain involved nor the importance of the knowledge to be obtained. Says President Eliot of Harvard University:

> The humanity which would prevent human suffering is a deeper and truer humanity than the humanity which would save pain or death to animals.

Moreover, the experiments give knowledge which saves pain not only to millions of human beings, but in many cases to animals themselves. In tuberculosis, for instance, the men of science are fighting for cattle as well as for men; in lockjaw, for horses as well as for our own kind. The marvelous results in diphtheria have happily now become known to almost every mother. To stop animal experimentation would check the advance of surgery. It would take away our strongest weapon in the promising fight being waged against cerebro-spinal meningitis, bubonic plague, dysentery and malaria. It would reduce us to despair in the harder but still hopeful contest with cancer.—*Collier's Weekly.*

SCIENTIFIC BOOKS

The Manufacture of Explosives—Twenty Years' Progress. By Oscar Guttmann. 8vo, 84 pp., 11 illustrations. New York, The Macmillan Company. 1909. Price $1.10 net.

The major title of this book is identical with that of the well-known two-volume work by the same author which was published in 1895. The make-up of the new volume is similar to that of the older ones and it may properly be regarded as a supplement to them. The significance of the subtitle is not apparent on a close reading of the text, for the first installment begins with an historical résumé from 1250 to 1886, and this same method of treatment obtains throughout the book as new topics are introduced. Even taking 1886 as the point of departure, this date precedes the publication of the first volumes by nine years, so that there is necessarily some repetition in the supplement, but much of it is avoided by referring to the descriptions published in the earlier volumes. Nevertheless, this feature should be borne in mind when citing this author in litigation or for historical precedence and the statements of the supplement should be carefully compared with those of the major parts.

This condition has arisen from the fact that the present volume is a record of four Cantor lectures delivered before the Royal Society of Arts in 1908 and that such historical résumés were deemed necessary to properly introduce the topics. Lecture I. deals with black powder and other nitrate mixtures, chlorate mixtures, "metallic" explosives, picric acid, picrates and trinitrotoluol; lecture II. with nitroglycerine, dynamites, guncotton and nitrostarch; lecture III. with smokeless and flameless powders, fulminates, detonators and fuses, safety explosives and their use, particularly in mines; lecture IV. with the use of nitrocellulose in other industries, the construction, lighting and inspection of factories, accidents and precautions to be taken, the merits and demerits of explosives, stability of explosives and stabilizing agents, and finishes with a prophecy regarding the powder of the future.

The author holds a very poor opinion of nitrocellulose as a material from which to make smokeless powder, though all smokeless powders now adopted for military and naval use are composed of nitrocellulose alone or mixed with nitroglycerine, and he predicts that a stable nitro-compound of the aromatic series alone, or in conjunction with nitroglycerine, will come into use so soon as some government finds the courage to make the change. He likewise regards picric acid, which has been adopted by almost every country as a disruptive agent, under names such as melinite, lyddite, shimose powder, ecrasit and others, as a treacherous substance and expresses the hope that we shall some day give up its use.

Considering the use of explosives in mines, he points out the difficulty of determining what makes an explosive safe in fire damp. Thus since mercuric fulminate ordinarily does not ignite fire damp, while black powder does the Prussian Commission state that the more rapid the explosion the safer the explosive, yet certain black powder mixtures like bob-

binite are safe up to a certain point while nitroglycerine and blasting gelatine are not. The French Commission decided that an explosive whose temperature of explosion, as calculated by certain thermochemical data, was below 1,500° C., could be licensed for use in fiery mines, yet carbonite, which is one of the safest of all, and several others in use, have a temperature of explosion considerably above 1,500° C. Bichel and Mettegang, whose investigations in this field are highly praised, require slow detonation as one of the characteristics of a safe explosive, yet "the velocity of detonation can not, however, be considered to be a determining factor under all circumstances. Certain nitroglycerine explosives, amongst which we may also include carbonite, explode much more rapidly than, say bobbinite, and yet show themselves to be much safer when tested." Even in making firing tests in galleries, as is now being done by several governments and organizations, the author finds that the results differ with the shape and dimensions of the galleries, so that each gallery may have its own ignition temperature which would affect the results obtained.

In discussing this topic the author says: "It has been known for a long time that coal dust as well as pit gas is highly explosive," while he knows perfectly well that neither coal dust nor pit gas is explosive by itself, though they may form explosive mixtures with the air. Also in discussing "smokeless," "flameless" and "safety" explosives he fails to point out that these terms are used in the art in a purely relative sense and that an explosive possessing these purely negative qualities absolutely does not exist. Justice requires us to state, however, that when discussing catastrophes in explosive works he says: "The author has always warned manufacturers and users alike that the function of an explosive is to explode, and that although certain compositions are almost insensitive to ordinary impulses, such as blows, friction, etc., yet he never believed that any explosive existed which under favorable conditions and by proper means could not be made to explode," but this point is emphasized because in a publication such as this, which may be cited as authority in litigation in which important interests are involved, care should be taken that no loose terms or unqualified phrases regarding the properties of matter are used.

Surprise is expressed at the extent to which black powder is still being used, it being stated that 7,000 tons of it were used in the mines and quarries of Great Britain, and 3,597 tons exported in 1907, making 10,597 tons in all. This is markedly less than the output of the United States, where the production of black powder at the census of 1900 was 62,412 short tons and at that of 1905 was 107,910 short tons. In fact, since the statistics of this industry in the United States were first separately taken, at the census of 1840 there has been a constant increase at each decade, and this failure of "smokeless" powder to supplant black powder was commented on with fullness in the Census Bulletin for 1900.

The book is filled with information, much of which is quite up to date, and it bristles with references, a large part of which are to British patents. A defect is in its limited use of American sources, patent or other readily accessible literature, for a country which produced 363,748,097 pounds of explosives of all kinds in the census year 1905 can not have failed to have made useful contributions to the progress of the art. Nevertheless, the book is a good one. It is more than a compilation, for it is thoughtful, critical and sometimes controversial. Every one of the many who possess the parent volumes must also acquire this and they will be pleased to have done so.

CHARLES E. MUNROE

Birds of the World. By FRANK H. KNOWLTON, Ph.D., with a Chapter on the Anatomy of Birds by FREDERIC A. LUCAS. The whole edited by ROBERT RIDGWAY. With 16 colored plates and 236 other illustrations. American Nature Series, Group 1, Natural History, pp. i–xiv, 1–874. New York, Henry Holt & Co. 1909.

In the elaborate and excellent American Nature Series, now in course of publication, Knowlton's "Birds of the World" forms the third issue under the head of "Natural History," having been preceded by Jordan's "Fishes" and Kellogg's "American Insects."

Writers of bird-books sometimes hear the remark in tones of disparagement: "But you describe so few birds! The robin and the chipping sparrow are very well in their way, but we should like to read about the bird-of-paradise, and the toucan as well." Such critics will have no case when they handle Mr. Knowlton's weighty volume, for in this treatise he reviews the entire class; there are over eight hundred large pages, and these are embellished with nearly two hundred and fifty illustrations.

When we reflect that the birds of the world comprise upwards of twelve thousand species, and that the major and minor celebrities form a great company, with an already vast and ever-growing literature, the labor and difficulty of preparing a well-balanced hand-book for the entire field, which shall be both useful and interesting, is a task of no holiday order. In our opinion the author has succeeded admirably in nearly every respect. His work, from necessity a compilation, is certain to prove very useful; it is published in attractive form, and possesses a large fund of interesting facts concerning the structure, classification and general habits of the better known and more important members of the entire avian group.

The body of the work is preceded by several sections of an introductory nature, dealing with the general characteristics of birds, with their distribution, migration and zoological relations. This part further includes a very compact and valuable chapter on the Anatomy of Birds by Mr. Frederic A. Lucas. The fact that the entire work has passed under the "editorial censorship" of Mr. Robert Ridgway should tend to discourage those critics whose appetite is whetted by their ability to find mistakes. Twenty-one chapters follow on the "orders" in the classification adopted by the author, beginning with the sole occupant of a subclass, the famous *Archæopteryx*, which has been pronounced three fourths "bird" and one fourth "reptile," and ending with the great order of Passeriformes, said to embrace over seven thousand species, or more than one half of all known birds. This is followed by a full, and therefore useful index.

Dr. Knowlton's book is presumably not one of the kind intended to be read from cover to cover, but like Newton's "Dictionary of Birds" and other extended treatises, is to be used as a compendium for reference. The author has surveyed the most pertinent literature so well, and has preserved so sane a judgment in dealing with it, that he is a guide to be trusted on all matters of which he treats. His errors will be found to be chiefly those of omission.

Some criticism could be made of the author's style, but this is certainly even and clear, and probably well adapted for a reference-work of this kind. The reviewer has found no errors of consequence, those noted being of a rather trivial order, as when in speaking of the night-hawk, Mr. Knowlton says (p. 43): "Some individuals" spend "the summer in Alaska and the winter in Patagonia," which may be true, but is an assumption, to be tested only by the marking of individual birds. The "banding" or "ringing" of individuals to obtain exact data on migration, first tried with a measure of success on the *Phœbe* by Audubon, has been taken up rather recently, and with the promise of yielding very valuable results.

We miss from the introductory matter any summary of the modern work on the instincts, intelligence or general behavior of birds, a subject no longer of interest to psychologists only. In speaking of the absence of "hook-like processes of the ribs" in *Archæopteryx* as a "decidedly reptilian character" it should perhaps be noted that the New Zealand lizard-like *Hatteria* possesses them, as well as the crocodile and alligator, and the latter, moreover, builds a nest which is guarded by the male.

We can not subscribe to the view that "we are without an adequate theory of birds'

nests," for we regard the nesting instinct as no more inexplicable than the other instinctive actions which recur with almost clock-like regularity in the reproductive cycle of most birds. The entire round of activities which leads to the production of a certain type of nest, as in the robin, vireo, or oriole, is without doubt remarkably uniform and stable. But it is far less stable or uniform than the conditions which determine the form and color of the egg. If this is true it is not altogether surprising to find some open nests with snow-white eggs, and some closed ones, like the magpies', in which the eggs are spotted. Yet no one could maintain that the behavior of the wild bird is to be explained by any simple formulæ, at any point.

The abundant illustrations which have been drawn from a variety of sources, are naturally uneven in proportion and quality, half-tones of photographs from life having been excluded to keep down the weight, but the plan thus followed has certainly led to variety in abundant measure.

FRANCIS H. HERRICK

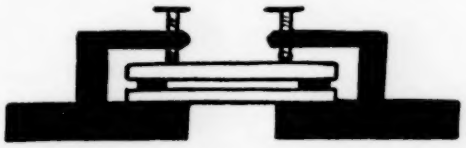
SPECIAL ARTICLES

A SIMPLE FABRY AND PEROT INTERFEROMETER

DURING a course of experiments with interferometers it was found that a very simple and inexpensive Fabry and Perot instrument could be constructed of plate glass which gives results almost as good as the costly interferometer. The construction of this apparatus for demonstration purposes will well repay the teacher and student. The sharp-colored interference rings obtained by using luminous gases in vacuum tubes as sources are extremely beautiful. The D lines from a sodium burner are easily separable. If the interference pattern using a copper or iron arc is focused on a wide slit of a single prism spectrometer, a section of the interference rings is seen in the various spectrum lines, illustrating the method of Fabry and Buisson for the determination of the new standard table of wave-lengths. The Zeeman effect can also be easily shown with this apparatus.

Take two pieces of plate glass about an inch square (I have used the so-called German plate) and silver[1] them till one surface of each plate cuts down the intensity of the transmitted light to about a quarter of the incident light. Separate these silvered surfaces by two strips of cardboard. A useful thickness to begin with is the cover of the 24 two-cent postage-stamp book, as this will clearly separate the D lines. Mount these plates over a half-inch hole in a metal plate by means of three pressure screws, two of which are shown in the following diagram, being a section through the center. The third screw is midway between the other two and at the end of the plates.

Looking normally through the plates at the glowing filament of an incandescent lamp, a number of images of it will probably at first be seen. Adjust the pressure screws until these images are in juxtaposition in the line of sight; the silvered surfaces are then approximately parallel. Place the instrument in a clamp stand, and focus the light from a sodium flame or a vacuum tube upon the plates and look at the interference bands with a small laboratory telescope focused for infinity. Usually the eyepiece has too large a magnification for the above retardation and it is better to use in place of it a single lens



of focal length about two inches. At first only a small section of the interference pattern is seen, but with a little careful adjustment of the pressure screws the whole ring system is obtained in sharp focus. Removing the telescope and with the above lens used as eyepiece, focus the interference system from the above sources or an arc, upon the slit of a spectroscope. The bands in the different spectrum lines are then observed with the telescope on the spectrometer.

For further suggestions regarding the adjustments and other experiments for which this apparatus can be used, refer to an article

[1] For silvering solutions see the appendix to Baly's "Spectroscopy."

by the writer in the *Philosophical Magazine* for May, 1904.

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SOME COMMENTS ON THE REACTIONS OF PERICHÆTA

In a discussion of the method of trial in Science, Vol. XXVI., 662, Professor H. B. Torrey referred to the writer's description of the light reactions of *Perichæta*. He interpreted the behavior of *Perichæta* in weak light as displaying "Unterschiedsempfindlichkeit" and not the tropic reaction, with the resulting conclusion that there would be no orientation in weak light. The writer had stated that *Perichæta* responds to weak light chiefly when the anterior end is extended, presumably because of the greater exposure of photoreceptor cells in the integument. Torrey called these "Unterschiedsempfindlich" reactions because apparently due to an increase in the intensity of the light on the cells. Loeb first introduced the distinction between the tropism as a constant stimulus effect and the reaction to change of intensity in the case of *Serpula*, which bends towards the light and also withdraws suddenly into its tube from the stimulus of a shadow cast upon the oral end. Except for the opposite sign of the heliotropism the reactions of *Perichæta* and this other annelid bear a family resemblance. I need only refer to the familiar facts that *Perichæta* or *Lumbricus* turn away from all but the weakest light and retract into their burrows on sudden illumination. In the open, the worm gives the "Unterschiedsempfindlich" reaction of retracting its head on sudden illumination with strong enough light and after a period of backward creeping follows this up by a tropic, turning response effects of sudden illumination are conspicuously wanting in weak light, as ordinarily only turning movements appear.

A form of response to illumination of the anterior end which is between these extremes consists of creeping backward after a distinct pause, which is often prolonged, and without any sudden movements which would naturally be related to the change produced by the stimulus. These weaker responses might naturally be regarded as constant stimulus effects. Reactions attributed to change of intensity ought to give manifest evidence of the shock in resulting movements or inhibitions.

The objection is raised that the transitoriness of the stimuli in weak light, received during extension movements, would preclude their giving rise to orientation. The tropism is ascribed to a differential tonus produced on the muscles of the two sides. There is no apparent reason why even transitory light stimuli of any intensity might not produce some appreciable tonic effects. It appears, however, that a considerable change of intensity is required to temporarily inhibit forward movement, as is the case in retraction of the head. If the sudden manifestations of shrinking are absent in weak light is it not apparent that the threshold for "Unterschiedsempfindlich" effects is higher than for purely tonic, *i. e.*, tropic effects in the earthworm? As for the application of the trial hypothesis to the behavior in weak light, that is only giving a name to the somewhat gradual process of orientation, interrupted by movements contrariward which are less influenced by the light.

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ENTOMOLOGICAL CONFERENCE ON THE PACIFIC COAST

The department of entomology of the University of California has for several years past held four conferences during the school year at stated intervals, the place alternating with Berkeley. Thus during the last school year four such conferences were held, two at Berkeley, one at Watsonville and another at Davis. The last of these meetings, held in Berkeley, was planned to be more inclusive, inasmuch as entomologists from the entire Pacific coast were invited to attend and present papers. The hope was also expressed that a special organization of western entomologists might be effected, inasmuch as the insect problems of the Pacific slope are so different from those on the other side of the Rocky Mountains.

At this meeting, held April 20 to 23, the following general program was carried out:

Tuesday afternoon (April 20), Lime Sulphur, Its Use and Manufacture. Evening, The Manufacture of Miscible Oils and Arsenical Insecticides.

Wednesday morning (April 21), The European Elm Scale, and the Codling Moth. Afternoon, The Orange Scale and the Citrus Mealy Bug. Evening, Exhibits of Insecticide Materials, Insect Collections, Apparatus illustrating Methods of Study, etc.

Thursday morning (April 22), Visit to Oakland Formicary. Afternoon, Forest Insects and Apiculture. Evening, Medical Entomology.

Friday morning (April 23), Methods Used in the Study of Sensory Reactions, Insect Photography. Afternoon, permanent organization.

The meeting was well attended, notwithstanding the enormous distances separating the workers on the Pacific coast. As had been hoped at the outset, a permanent organization was effected under the name Pacific Slope Association of Economic Entomologists. The constitution adopted requires that active membership shall be limited to the official and professional entomologists of the Pacific slope, while associate membership shall be open to agriculturists and to all others interested in the objects of this association. The following officers were elected:

President—Professor C. W. Woodworth, University of California, Berkeley, Cal.

Vice-presidents (representing each state concerned)—Professor R. W. Doane, Palo Alto, Cal.; Professor S. B. Doten, Reno, Nevada; Professor J. Elliott Coit, Phœnix, Arizona; Professor Fabian Garcia, P. O. Agricultural College, New Mexico; Professor E. D. Ball, Logan, Utah; Professor A. B. Cordley, Corvallis, Oregon; Professor A. L. Melander, Pullman, Washington; Professor L. F. Henderson, Moscow, Idaho; Professor C. P. Gillette, Fort Collins, Colorado; Professor R. A. Cooley, Bozeman, Montana; Professor Aven Nelson, Laramie, Wyoming; Hon. Thos. Cunningham, Vancouver, B. C.

Executive Committee—Mr. R. R. Rogers, San Francisco, Cal.; Mr. H. P. Stabler, Yuba City, Cal.; Mr. L. H. Day, Oakland, Cal.

Secretary-Treasurer—Professor W. B. Herms, University of California, Berkeley, Cal.

It is planned to hold the next meeting this summer at Portland, Oregon.

W. B. HERMS,
Secretary-Treasurer

SOCIETIES AND ACADEMIES

THE PHILOSOPHICAL SOCIETY OF WASHINGTON

THE 667th meeting was held on May 22, 1909, Vice-president Wead in the chair. Mr. Edwin Smith read a biographical sketch of Mr. William Eimbeck. Two papers were read.

Investigation of Dip Needle Corrections by Experimental Methods: P. H. DIKE, of the Carnegie Institution of Washington.

The values of the inclination, or dip of the magnetic needle, as observed by the absolute method with the dip circle, in general still require some correction, and the error is not eliminated by multiplying observations as the correction is a constant one for a given station. The correction is found to vary with the dip and the total force, and it is accordingly necessary to take account of this variation in the reduction of the observations made by the various expeditions of the Department of Terrestrial Magnetism of the Carnegie Institution of Washington covering a large range of the magnetic elements. In the *Galilee* work on the Pacific Ocean dip circles were compared with observatory instruments over as wide a range of dip as possible ($+74°$ to $-68°$), and from the corrections thus obtained empirical formulæ were established by least square methods from which the probable corrections for intermediate values of dip were derived.

The purpose of the present investigation is to derive these corrections by comparison with a single standard instrument (an earth inductor) at one station and through the whole range of positive and negative dip. An artificial field is produced by a system of coils, through which a uniform current from a storage battery flows. Two coaxial coils of 80 cm. radius are mounted with axis vertical, 80 cm. apart, each coil having 100 turns of wire. A second pair 90 cm. in radius and 90 cm. apart are mounted with axis horizontal, the middle points of the axes of the two pairs of coils coinciding. The second pair has 50 turns each of wire. By regulating the currents in the two sets of coils any desired magnetic field can be produced at the center, and this field is extremely uniform over a considerable area. Two systems of coils are set up about 50 feet apart, the horizontal coils of one being in series with those of the other, and the vertical coils likewise in series with each other. Simultaneous observations with earth inductor and dip circle are made

at every 20° of dip from + 80° to — 80°, with interchange of instruments on each dip, to eliminate station difference. Curves were shown representing the corrections thus determined for the four needles of a Dover land dip circle recently purchased, which is found to be an exceptionally good one. The corrections range from + 0′.5 to — 2′.8. The similarity of the curves for the four needles suggests that part of the error is inherent in the dip circle rather than in the needles. The station difference showed a regular variation with the dip, following approximately a sine curve. Possible variations in method and sources of error were discussed.

Ship dip circles in general exhibit a considerably larger variation in the correction with varying dip and force than the land instruments.

The Carnegie Institution Marine Collimating Compass: J. A. Fleming, of the Carnegie Institution of Washington.

The experience gained on board the Magnetic Survey yacht *Galilee* indicated the necessity for developing an instrument of greater precision for determining magnetic declination at sea. The instrument exhibited was devised by Mr. W. J. Peters, who is in charge of the ocean magnetic survey work of the institution.

The compass is of the liquid type. Four spherical mirrors are attached to the buoyant air chamber at intervals of ninety degrees, their optical centers being in the horizontal plane through the point of suspension. In the focus of each mirror there is a scale of thin, blackened German-silver wire. The optical arrangements are such that homocentric rays from the scales are reflected through the dilute alcohol in which the compass is mounted and issue as parallel rays into the air through windows exhibiting the scales each as seven luminous points one degree apart. The windows are segments of spherical shells, the centers of which are at the point of suspension so that the optical conditions are not altered by the rocking of the bowl and other motions at sea. The bowl swings in a perforated gimbal ring supported in a spindle-bearing cylinder with graduated base, thus making it possible to quickly orientate the optical systems. The usual form of binnacle stand with suitable alterations is used.

The method of observation calls for the measurement of the angle, a, between the collimator and a celestial body of known altitude, h. As the angle, c, between the zenith and the collimator is ninety degrees, the fundamental formula takes the form $\cos A = \cos a \cdot \sec h$ where A is the magnetic azimuth of the celestial body. (For small values of A more accurate results may be obtained by use of formula involving the tangents of half the sum and difference of a and h.) Errors in c due to rocking may be eliminated by suitable arrangement of a series of observations. Lack of perfect adjustment of collimator producing error in c of dc which may be easily measured and the resulting error in A corrected by $dA = -\cot B \cdot dc$, where B is the angle between the vertical plane and the great circle passing through the collimator and the celestial body. Owing to the trigonometrical relations involved, the conditions are not good for values of a less than 45° or more than 135° for the usual upper limit of altitude of 15°; it is for this reason that four collimators are used, as then suitable selection may be made.

A special instrument for measuring the angle a has been constructed, although the usual form of pocket sextant may be used for this purpose. This instrument, which may be called a circle of reflection, makes use of the law that the incident and reflected rays lie in the same plane normal to the mirror surface; the construction is such that the angular motion of the mirror is equal to the angle measured.

The advantages are: motion is practically restricted to the oscillation of the magnets; the celestial body observed upon and compass scale are seen simultaneously and all the observed quantities refer to the instant of observation; there are no movable parts subject to wear; graduation errors are limited to twenty-eight divisions, a number so small that each may be separately examined; error of eccentricity affects only the distance between mirror and corresponding window, hence the focal distance and consequently the scale values, but does not alter the constant for the middle of each scale. By the attachment of a suitable standard centrally to the bottom of the bowl horizontal intensity determinations by the method of deflections introduced by Dr. L. A. Bauer can be made and from the same measures accurate values of declinations may be derived.

The instrument is not intended for navigational purposes, but it is hoped that it may give much greater precision in the determination of declination at sea for use in the more careful study of the laws governing the distribution of magnetism over the globe.

R. L. Faris,
Secretary